DOCUMENT RESUME

ED 394 318 FL 023 721

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TITLE The Value of Research and Evaluation in Language Education.
INSTITUTION Council for Cultural Cooperation, Strasbourg (France).
PUB DATE 29 Jan 96
NOTE 31p.; Keynote address at the Educational Research Workshop on the Effectiveness of Modern Language Learning and Teaching (Graz, Austria, March 5-8, 1996).
PUB TYPE Viewpoints (Opinion/Position Papers, Essays, etc.) (120) -- Speeches/Conference Papers (150)

EDRS PRICE MF01/PC02 Plus Postage.
DESCRIPTORS *Evaluation Research; Foreign Countries; Hypothesis Testing; Information Needs; Language Research; Needs Assessment; *Policy Formation; *Research Problems; Scientific Methodology; *Second Language Instruction; *Second Language Learning; Verbal Development

ABSTRACT

This keynote address argues that policy makers should consider a project approach to implementation of research results regarding foreign language learning; they should pay attention to what is being revealed by a growing number of evaluation and impact studies concerning effective innovation and project effects. Second, language teaching research needs to develop better definitions of linguistic and non-linguistic outcomes if research is to address the complexity of the task of foreign language learning. There appears to be a pervasive lay belief in the essential simplicity of the task of learning a foreign language, encouraged by the mistaken notion that foreign language learning and first language acquisition are comparable. Such beliefs may partially explain the commercial, if not educational, success of teaching methods and materials that make claims of effectiveness in remarkably short periods of time. Careful research has failed to bear out such claims or to support such beliefs. Even if effective approaches were known, it is naive to believe that research results can be translated into action without consideration of the conditions under which innovations take hold and succeed. The language teaching profession has failed to define adequately what it means by success in language learning; there is little agreement on expected or desired standards of achievement. There is also little attention to the principles that are accepted in the language testing profession for the construction of measures of achievement and proficiency. Language teaching research will need to develop much better definitions of the linguistic and non-linguistic outcomes that are expected of the various programs and projects if research is both to address the complexity of the task of foreign language learning as well as to contribute to our understanding of the conditions that can lead to successful language learning. (Contains 58 references.) (Author/NAV)

Council of Europe
Conseil de l'Europe

ED 394 318

Strasbourg, 29 January 1996

DECS/Rech (96) 6
Or. Engl.

COUNCIL FOR CULTURAL CO-OPERATION

Educational Research Workshop on the effectiveness of modern language learning and teaching, Graz (Austria), 5-8 March 1996

THE VALUE OF RESEARCH AND EVALUATION IN LANGUAGE EDUCATION

Keynote Address

by

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Abstract

There appears to be a pervasive lay belief in the essential simplicity of the task of learning a foreign language, encouraged by the mistaken notion that foreign language learning and first language acquisition are comparable. Such beliefs may partially explain the commercial, if not educational, success of teaching methods and materials that make claims of effectiveness in remarkably short periods of time. Careful research has failed to bear out such claims or to support such beliefs.

The amount of research into language teaching and learning has grown enormously in recent years, so much that it is increasingly difficult to gain an overview of what the results are telling us. Moreover, the studies have failed to have much impact on the teaching profession and on materials, for a number of reasons.

One is the lack of academic respect for research into language teaching as compared with the prestige of research into language analysis or literature and culture. Another is the lack of national coordination, direction and focus which results in rather inadequate small-scale studies, often by doctoral students, which fail to be replicated. A third lies in the lack of research which addresses teachers' concerns and which is written in such a way as to be accessible to them. A fourth is the lack of adequate means of disseminating the results we have.

Even if we knew which approaches to language teaching were effective, however, it is naive to believe that the research results could be translated into action without consideration of the conditions under which innovations take hold and succeed. A better understanding of innovation theory is recommended, as is consideration of the literature on project design and evaluation. It is argued that policy makers should consider a project approach to implementation, and to this end should pay attention to what the growing number of evaluation and impact studies are beginning to reveal about effective innovation and project effects.

The language teaching profession has on the whole failed to define adequately what it means by success in language learning, and there is little agreement on expected or desired standards of achievement. There is also little attention to the principles that are accepted in the language testing profession for the construction of measures of achievement and proficiency. Language teaching research will need to develop much better definitions of the linguistic and non-linguistic outcomes that are expected of the various programmes and projects if research is both to address the complexity of the task of foreign language learning as well as to contribute to our understanding of the conditions that can lead to successful language learning.

However, research into second language acquisition, into language classrooms, and into the motivating effects of the interaction between the learner and the learning task within specific environments already holds some promise for such a goal.

1. THE NEED FOR RESEARCH

Most of my readers will have seen exaggerated claims for the success of particular language teaching courses. My most recent experience was an advertisement, in the Sunday Observer of December 31st. 1995. for Berlitz Language Courses: "The complete language course". The ad claims that the course "is designed to have you speaking. thinking and enjoying life like any native in any one of six countries in just one month. It works the way you learnt to speak as a child: gradually and naturally."

But unlike the way in which you learned as a child this course merely requires 30 minutes of your time for 30 days. And unlike your childhood experiences, this one costs money: up to £115 for the CD version.

I was intrigued by the outrageous. but carefully phrased claims. Note that lawyers could doubtless argue that there is no commitment as to which country's native you will resemble after one month, and that they could also argue that enjoying life like a native is perfectly possible (even without spending £115). So I wrote to the distributors, expressed my interest and requested references to any evidence that might support the statement. I thought the evidence might be relevant to this Workshop. I have as yet received no response

Perhaps such claims are so obviously silly that the advertisers do not risk prosecution for purveying falsehoods. But somebody must believe them, since the products sell, even at such prices. Indeed, the general public, at least in Britain, shows considerable determination to learn foreign languages, and buys enormous quantities of course books, tapes and videos, watches language courses on TV, and attends numerous evening and leisure classes. Usually without the success predicted by the salestalk. There is, I think, a pervasive belief that language learning actually is straight forward, even easy and painless, if only we could find the right method, the right materials, the right video, or these days, computer assisted package. Unfortunately, when students meet with lack of success, they are more likely to blame their own shortcomings rather than the complexity of the task or the silliness of the advertising.

Hence perhaps the claims, and the publicity for 'methods' like Suggestopaedia, which seem to endure in the face of professional criticism and research. Tom Scovel wrote an excellent review of the English translation of Lozanov's book 'Suggestology and Outlines of Suggestopedy' in 1979. He details the absurd claims Lozanov makes for the method:

"Suggestology is a hyper-rapid learning technique which - by making possible the assimilation and comprehension of up to 1,000 new words daily - is revolutionising the teaching of foreign language".

"As seen from the results obtained in experimental groups, memorization in learning by the suggestopedic method is accelerated 25 times over that in learning by conventional methods" and so on and on. Scovel reports that descriptions of the method in popular magazines resulted in friends and neighbours asking him to urge the adoption of suggestology by language departments in his university. The public is ever gullible, as Scovel goes on to show.

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He examines the evidence for the assertions, and finds it misleadingly presented and grossly exaggerated. I quote:

"Suggestopedy, taken as a self-contained method for language instruction, offers at best nothing much that can be of benefit to present day, eclectic EFL programs, and at worst nothing more than an oversold package of pseudoscientific gobbledygook" (1979:258)

In EFL/ ESL, Suggestopaedia enjoyed a brief heyday, despite Scovel's review, but was presumably soon found to be the gobbledygook Scovel exposed, as its ridiculous claims could not be substantiated by teachers, and it has more or less disappeared from the literature, conferences, and even sales exhibitions. Yet the misleading assertions continue, and recently, I came across a study of the effectiveness of Suggestopaedia for learning French (Schlick-Renner and Truscott, 1994). Quite why the authors had not read the research literature, I do not know - perhaps because it was American, and dealt with Suggestopaedia applied to English and not to the language they were interested in. They appear to take the method seriously, since they attempted to test its claims, and organised two French courses for adults, one an intensive course, the other a 10-week course. Sadly, they do not have any control group or parallel course, and their study only reports class atmosphere, student opinions and drop-out, and says nothing about outcomes. The study nevertheless concludes that such courses can only be enjoyed by outgoing personalities, and that more introverted people can find the approach distasteful. "It is not clear that this method would work with true beginners. (The) materials are both interesting and motivating, but aggressive marketing and exaggerated claims of success are counter-productive." Let us hope so, although this revival of interest in a discredited method suggests not.

Why are such claims made? Why do people continue to treat them seriously? What evidence is there for the effectiveness of particular courses, methods, techniques, approaches to foreign language teaching and learning?

2. BELIEF AND PREJUDICE

I suspect that part of the reason lies in a whole set of beliefs and prejudices about language and language learning shared by the general public, and encouraged by unscrupulous commercialism. These beliefs are along the following lines:

- Everybody learns a first language therefore language learning must be straightforward.

- First language learning is easy, so successful second language learning must be based on what young children do.

- Young children imitate and practice, they speak and listen before they read and write, they pay no attention to grammar, and so on. That's how we should learn foreign languages.

- Since we all speak languages; we all know about how to learn them. Any native speaker is capable of teaching non-natives how to speak their language.

- First language learning is so common yet foreign language learning is so rarely successful: what makes it so hard? All we need to do is to find the key - the right method, the correct theory

I shall not respond to such statements, but I am sure my readers will have their own reactions. The point I am making is not that they are true, but that they are widely held.

Steven Pinker has recently propagated a theory that language is an instinct, which he claims accounts for the ease of first language acquisition (Pinker, 1994). He bases his argument on Chomsky's notion of Universal Grammar, and claims that as human beings we are simply programmed to learn language. There is a sense, unfortunately, in which this claim reinforces the belief that there is nothing to language learning. Such a conclusion would, however, be absurd: being instinctive does not make language learning less complex. Its complexity makes it a remarkable achievement, and especially for foreign language learning by adults who have already developed a language competence.

The very complexity of foreign language learning leads to the fact that it is rarely as successful as first language acquisition. This is turn leads to feelings of frustration and a sense of inadequacy, which is preyed upon by those who make exaggerated claims for their products or methods. And, of course, the commercial importance of language learning, and especially of English in the late twentieth century, leads to claim and counter-claim for materials, methods and media.

Morever, the commercial and political importance of foreign language teaching, especially of English, leads to political involvement: in the promotion of English by the English-speaking nations, and the counter action of nations that do not speak English: the promotion of the 'lesser commonly taught languages', the protectionism of French. Foreign language teaching, therefore, is too important a matter to be left to the charlatans who would have us believe it is a simple matter, if only we bought the right product, or followed the correct method.

Yet the search for Methods that Work (the title of a book edited by Oller and Richard-Amato that makes claims unsupported by empirical evidence, and that sold well) continues unabated, to the surprise of Janice Yalden, who reports (Yalden, 1995) her astonishment on emerging from a decade of administration away from language teaching to find that, despite her believing that the debate about methods had died years before her hibernation, it was actually alive, if not well. Note, for instance, the latest series of books for teachers from TESOL Inc: New Ways of Teaching Reading, New Ways of Teaching Speaking, and so on. Indeed, one might add, note the existence of this very Workshop on the Effectiveness of Foreign Language Teaching: does it reflect a belief that somewhere there might just be an elixir, if only we can do the right research?

Since the aim of the Workshop is precisely to explore the question of what the research tells us: I look forward to the results!

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3. WHAT RESEARCH HAS BEEN DONE?

My own approach to the task of writing this keynote address was instructive. I took the title of the Workshop to mean that I should review all relevant research and come up with some synthesising statements about what research tells us. In addition, I was told to make my statements relevant to policy makers: what sorts of policy changes or confirmations are implied by research results?

Although I have general awareness of what research has been done, and what research results might suggest, I was conscious that I have by no means a confidently accurate overview of the field, that my memory is less than perfect, that my recent reading in the literature has been patchy, and that necessarily I am more familiar with my own research field of testing and educational evaluation than with other areas of language teaching. I therefore decided that I ought to review the relevant literature.

Although a somewhat daunting task, I figured that this was what was expected of me. But where to begin? Skimming through the relevant professional journals on my shelves like TESOL Quarterly, Language Testing, English Language Teaching Journal, Reading in a Foreign Language, and aware that there were many more journals on the shelves in the University library - Applied Linguistics, Studies in Second Language Acquisition, Reading Research Quarterly, Language and Education, and others - fairly quickly convinced me that there was no way I could complete such a task within the allotted year, and carry on with my ordinary responsibilites as Head of Department, teacher and human being.

So I next considered reading state-of-the-art reviews, and abstracts in the abstracting journals: Language and Language Behaviour Abstracts, and Language Teaching. Indeed I made a start on this, and xeroxed all the possibly relevant state-of-the-art surveys since 1987, the list of whose titles is itself informative:

Vocabulary and second/foreign language teaching; Contemporary paradigms in syllabus design, Parts 1 and 2; Language testing, Parts I and II; The management of language training; English for academic purposes; Communicative theory and its influence on materials production; Reading in a foreign language at intermediate and advanced levels with particular reference to English; Pedagogical grammar; Computer-assisted language learning; Contrasting discourses; contrastive analysis and a discourse approach to writing; A decade of development; educational technology and language learning; Self -access; Learner language; Intercultural communication; Attitude change and foreign culture learning; A student's contributions to second language learning; Part 1; Cognitive variables; Part 2; Affective variables; English and other languages for younger children; practice and theory in a rapidly changing world; Self-managed learning; Evaluation and English language teaching.

This list does not include other state-of-the-art surveys that might have been relevant but which I judged to be less so: Business English, The Language of the Law, and several others.

Confronted by a pile of photocopies, I began my reading. Again rather quickly I came up with a problem. Not only was it difficult to remember all the results, and to synthesise them in any coherent way but many of the studies did not seem to provide evidence of the effectiveness of teaching, instead containing speculations about what might work: giving theoretical justifications and elegant and articulate argumentation, but failing to provide satisfactory evidence (or indeed in many cases any evidence at all) that what was being advocated did in fact 'work': was empirically justified. In a sense this came as no surprise, since it has long been my experience that in our field there has been a preoccupation with theoretical argumentation, armchair analysis and speculation and little or less interest in empirical verification or exploration of possibilities and suggestions. Where empirical evidence was available in the literature, moreover, the results did not appear to fall into clear patterns, and often there was contradictory evidence, depending upon the context in which the study had been conducted, the research design, the instruments used, and other variables. The authors were frequently able to give indications of useful directions in which future research might go, and even to discern trends in current and past research, but after reading several such surveys, my mind was again reeling, unable to see the wood for the trees.

I then decided instead to read the Annual Reviews of Research from the abstracting journal Language Teaching, the first four from 1987 to 1990 by Chris Brumfit and Ros Mitchell, and the next four - 1991 to 1994 - by Richard Johnstone. These were indeed interesting and relevant, but again after reading eight reviews, I found myself bewildered, confused, my head spinning, and not knowing what if anything to conclude.

At last, I came to my senses, and realised that I was trying to do far too much. Several colleagues had already been asked to provide reviews for this Educational Research Workshop on various aspects of the field of language teaching. I was not only giving myself an impossible task: I was potentially duplicating the work of others. With great relief, I decided on a different approach, the result of which is this paper.

However, the experience was highly instructive. Firstly , it convinced me viscerally (although I already knew it intellectually) that the field was very wide-ranging and the issues were many and complex. Secondly, it became painfully clear that no one person could hope to gain an overview of such complexity in anything like an 'objective' manner, and that necessarily one would be prejudiced, and influenced by what one could remember or what one had found especially interesting or relevant.

Above all, however, I was struck by the thought that if I, as an academic language professional whose job it is to teach and research in this field, and who, despite other administrative responsibilities, can devote large amounts of time, energy and attention to the topic of language teaching, found it impossible to come up with generalisable insights into the effectiveness of language teaching, then how much more difficult would it be for language teachers themselves, whose main preoccupation is necessarily with the actual teaching, and not the reading of the research?

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Note that this is not to conclude that the research is irrelevant to teachers, although it may be. I just found it hard to get an overview of what is or might be relevant to which teachers, and for what purpose. So, one issue that emerges is: how can teachers benefit from the research being done? I shall return to this issue.

4. THE STATE AND STATUS OF RESEARCH

During my reading I was struck by a number of other things.

Firstly, the CILT (Centre for Information on Language Teaching) Survey, summarised in Johnstone's Annual Reviews, dealt with research in progress, completed or about to begin in 1990-91; 1992; 1993. In 1991, there were 156 responses from Higher Education (HE) institutions to a questionnaire, from which it emerged that only 77 projects reported were dealing with research. The other 79 responses, according to Johnstone (1992), were for initiatives in either curriculum or materials development, which lead him to question the awareness of staff completing the returns as to what is distinctive about research over development activities. The funding devoted to research in the 1991 survey was about £651K, to curriculum development £758K and to materials development £613K. Materials and curriculum development are obviously also funded in other ways than through 'research', and in the year in question, of the 'research' projects, 32% were for research related to curriculum and materials development and 23% for evaluation of development initiatives. Only 18% was devoted to 'fundamental theoretical questions' (Johnstone, 1992: 147). Chambers (1992), discussing the results of the survey, argues for better coordination of future research and better funding, and Johnstone himself concludes this section of his 1992 Annual Review of Research thus: "It would follow that one of the first steps to be taken would be to raise awareness of what is distinctive about research as opposed to development, and what its benefits can be" (1992: 147).

Johnstone's next report of the CILT Survey (Johnstone, 1993) identifies funding arrangements as a major obstacle to long-term planning and, almost in confirmation of this, Johnstone (1994) reports a decline in new research projects identified by the CILT Survey from 77 in 1991 to 41 in 1992 to 21 in 1993. Morever, some of the 21 projects in 1993 were to do with language description and not language teaching and learning. It is worth quoting Johnstone's speculation on reasons for this in full:

"Among possible factors related to the decline were a fall in the numbers of school teachers seconded for research degrees, major changes in the organisation and funding of initial teacher education which were running counter to long-term research interests, and the proposed direct control of all practical educational research funding by the Secretary of State through the Training Agency, thereby separating educational research funding from the indirect mechanisms for all other academic areas" (Johnstone, 1994:154)

I know of no similar surveys elsewhere in Europe, but it would be interesting to compare the situation in Britain with that in other European countries, where academic, or even professional research into language teaching is likely to have similarly low status and funding.

Many HE Departments of foreign languages in Britain and elsewhere in Europe are more interested in researching literature, or other aspects of culture (cultural history, the film, popular media, and so on). Language-related research is typically infrequent. However, even language-related research is more often analytic or descriptive than applied to language teaching or learning. This lack of interest in applied linguistic research has often been commented on, and may well relate to the low status in many institutions of language teaching itself. Literary, cultural and linguistic studies are acceptable: applied linguistic research relevant to language teaching appears not to be respectable in many contexts. This must have some effect on our ability to understand what makes language teaching effective in many settings.

According to Jonstone, then, many 'researchers' active in HE have little idea what 'research' into language teaching and related matters actually means. Language teaching research is undervalued, underfunded, underemployed underutilised and not understood. Most money goes on materials and curriculum development, and the number of research projects is declining.

Note that the situation described in Johnstone probably applies even more to EFL in the UK than it does to modern foreign languages. At least modern foreign languages form part of the National Curriculum, and in theory are available for research funding and other forms of support. Although most universities have departments of modern foreign languages, there are few if any publicly funded Departments of English as a Foreign Language(EFL) in UK universities. Although the study of EFL is undertaken in some Education, English or Linguistics Departments, most academic work is undertaken in self-financing institutes often associated with such Departments and, since they are self-financing, they have to derive their income typically from language teaching and in-service teacher education, rather than from research.

English as a Foreign Language is not part of the English National Curriculum, the Department for Education and Employment does not recognise or provide EFL qualifications, and only minimal funding for research into EFL is available through public, quasi-public or private sources. The British Council, which promotes EFL abroad, has no money for research; the Overseas Development Administration only exceptionally funds research projects in language; and bodies like the examination groups have minimal research budgets and only fund applied research in very limited areas. Commercial publishers do occasionally fund research, most notably into computer-corpus building and exploitation for lexicographical purposes, but virtually never do they fund research into language teaching and learning.

The British Government has in recent months recognised that British English is a major income generator for the UK, through courses and coursebooks, examinations, and the associated tourist and hospitality industries. The Foreign and Commonwealth Office has explored ways in which funding might be made available to develop and encourage EFL, and the British Council has been encouraged to develop a project, known as 'English 2000', to promote British ELT. Yet, although the aim of these initiatives is to increase the marketability and awareness of British English and associated products, no funding has to my knowledge

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been earmarked for research into the effectiveness of the teaching of EFL, no discussion has been held of what sort of research might be needed in order better to support commercial development, nor indeed has any survey been commissioned into what research already tells us on this topic.

It is all the more remarkable, therefore, that any research into EFL takes place at all in the UK. In fact, the majority of this research is conducted by postgraduate students, many of them overseas students, either in Masters theses which are rarely read outside the institution, or in more extensive but still necessarily limited PhD research. However, such research lacks the benefit of a nationally or even institutionally coordinated research focus or direction, as Peter Skehan has noted for language testing in his state-of-the-art survey article (Skehan, 1988). One notable exception to this is the work of Paul Meara and his students in Swansea, where a conscious effort has been made to concentrate on research into vocabulary matters, but this programme is relatively new and the benefits of its focus have yet to materialise.

One consequence, then, of research into language teaching being largely undertaken by doctoral students is a lack of coordination and focus, and a plethora of disparate research studies in a wide range of areas. National policies on research priorities in language teaching are long overdue, and the identification of such priorities would be a major, albeit doubtless controversial, outcome of this Council of Europe workshop.

The 'English 2000' Project supported by the British Council and mentioned above provides interesting confirmation for the view that research lacks focus and coordination. A questionnaire was targeted at 2,000 "leading ELT specialists around the world", and responses were received from 1,398 people of whom 1221 (81%) were overseas. Although the main focus of the questionnaire was on market research: on the role of English in the next century, how English will be taught and learned, and what the future holds for British ELT, questions were included of less commercial and national interest, some of which related to research. In answer to the question "Research in key areas should be nationally co-ordinated and evaluated in the country where I work", 28% agreed strongly, 50% agreed, and 13% had no view. Only 8.6 % either disagreed or disagreed strongly. (There were interesting regional variations: in most regions, roughly 80% of respondents agreed in one way or another, but in the European Union, this fell to 60%, and 26% had no view. The EU and the Americas shared the highest percentage of those disagreeing, at 12%. Is there more resistance to central 'control' over research in those regions?)

As might be expected, 74% disagreed with the statement that "Research is only valuable if it leads to potential commercial applications", 64% agreed with the statement that "ELT teachers are influenced in their choice of ELT products by the theories underlying them", 94% thought that "International collaboration in research on language learning is important for the long-term health of the EFL profession", and 75% agreed that "Greater commercial support for research is essential if British ELT is to remain competitive". In answer to the statement that "ELT research within British higher education should be organised and aimed at priority areas in the same way as research in the areas of science and technology", 80% agreed.

Finally a mere 2.4% disagreed with the proposition that "High quality academic research is essential if British ELT is to remain innovative".

So research is important, should be focussed and coordinated, should have greater commercial sponsorship, but need not have commercial application. Overwhelming support was shown for greater international collaboration in research, but sadly the survey did not elicit opinions on which directions the research ought to take!

Interestingly, however, even when research has been funded and published, it is rarely incorporated into language teaching materials. Moran (1991), for instance, reports that ESL coursebooks take little account of research into strategies for lexical inferencing. Commercial publishers, perhaps not surprisingly but nevertheless rather depressingly, would appear to be more influenced by what they think markets want than by what research suggests would be effective. And the market is made up of teachers who themselves do not know the research literature, and who may be very conservative in their classroom practices. Indeed, recent research (Bonkowski, 1995) shows that teachers using innovative 'communicative' textbooks tend to ignore the innovative suggestions in the Teachers' Guides and follow rather traditional approaches, based upon their own intuitive understanding of what is involved in effective language teaching. What, then, is the value of research into effective language teaching, if it will be ignored anyway?

5. DISSEMINATION

Effective teaching is done by teachers, not researchers. Therefore at some point, teachers have to be reached by the results of research, if research is to have an impact on practice. If research is not addressed to teachers, how can it influence them? If research is not read, it cannot be acted upon. If research is not designed to be acted upon, it is unlikely to be.

These are familiar dilemmas, and are often used as the justification for the advocacy of action research. The argument in simplified form is that if research is to be acted upon by teachers it must be relevant to teachers and the best way to make it relevant to teachers is to get teachers involved in designing, doing and disseminating research. The arguments are familiar and I will not expand on them: I am in broad sympathy with the aim to engender relevant research that is acted upon. However, I am not alone in thinking that they are impractical: teachers are not researchers, do not see themselves as researchers, are too busy to do research, and do not think they have the skills to become researchers. Teachers may be wrong to hold such beliefs, and many writers on action research seek to disabuse them, but I believe they do hold them, and their tenure of the beliefs is strong and persistent.

My colleague Dick Allwright considers that a better notion than action research is that of exploratory practice (EP): where teachers are encouraged to identify 'puzzles' they wish to address and explore during their regular teaching. He has inspired a number of teachers in Brazil, Cuba and elsewhere to engage in such 'practice', and Sampson has summarised the results (Sampson, 1995). However, I fail to see a sustainable distinction between EP and

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Action Research, between puzzles and research questions, and I suspect that EP's achievements are more to do with the enthusism of activists, who would be interested in research anyway, in relatively well-resourced settings, than in the concept of EP, which I find ill-defined. I do not believe that EP addresses the fundamental problem: teachers do not see themselves as researchers or explorers, are not trained to be either, and above all do not have the time, the resources or even the motivation to research or explore. The fact that some teachers do do action research, do engage in exploratory practice is no more proof of the practicality or relevance of the notions than is the fact that some teachers do PhDs.

So, if the answer to the problem of getting research read and acted upon is not simply to get it done by teachers, what is the solution? One partially successful solution is consciously to disseminate research results to teachers. This is precisely what many professional journals do, like the AFLS Journal, TESOL Quarterly, ELTJ, MLJ, and so on. Inevitably, though, the articles are written largely for other researchers, not classroom teachers, and in any case are simply too numerous to be read on a regular basis by teachers, much less digested by them and turned into action. The journal Language Teaching attempts, with its summaries of published research and its publication of state-of-the-art reviews, to address the problem of availability and time, but I suspect its readership is still largely those interested in doing research anyway. More popular means of dissemination like TESOL Matters, or the more focussed publications and newsletters of the IATEFL SIGs are another way of addressing the issue, as are professional conferences like AFLS, IATEFL, TESOL, and regional variations of the same The work of CILT, in conducting surveys of research in progress, disseminating research results, and in running seminars on research into language teaching, such as the one recently completed at Cambridge in January 1996, is most worthy and notable, but I confess to doubts as to whether such work is reaching its presumed intended audience: average classroom teachers, whose practice CILT presumably hopes to influence.

An interesting recent development is the creation of electronic bulletin boards for teachers, of which the largest and most active in language education, with some 12,000 subscribers, is TESL-L managed from New York. These bulletin boards provide a forum for discussions on topics of interest to teachers, and one frequently encounters teachers asking questions on the network, to which a research study is an answer. Increasingly, too, the use of the Internet is likely to increase the ready availability of information, including information about research results, for those teachers who have a connection (and it is interesting to note that the British Council, as part of its English 2000 project, is establishing a Web service and considering setting up a bulletin board).

For most teachers, however, their professional organisation is likely to be more influential in the medium-term future than electronic sources of information. Teachers' organisations obviously have a major role to play, and the DELTA Handbook, produced by Dick Allwright and colleagues at Lancaster, constitutes a very useful guide to the setting up and running of such organisations. Sadly, however, as such organisations become large and successful, commerce takes over rather rapidly - the size and commercialism of TESOL is such that reseach is rarely presented or discussed at its annual conference, and researchers no longer find the atmosphere of hype and razzamatazz congenial to rational discussion of evidence for

effectiveness. Evidence suggests that the same commercial takeover is happening in other teacher organisations.

I have a suspicion that teachers' meetings on a national scale, whilst doubtless of value for many reasons, both motivational and commercial, are not the right venue for discussions that might lead to understanding of research and to change. I believe that teachers are more likely to be interested in innovation if it is explained or explored in terms that make sense to them, that are properly contextualised into their own realities and concerns. This is much more likely to happen at a regional or local level than nationally or internationally. One possible way forward might well be for local teacher support groups to be developed, preferably with official encouragement and resourcing, that would address focused issues and develop progammes for experimental action that was adapted to local circumstances and needs.

Thus I could envisage a group of teachers getting together to explore the research literature on ways of teaching vocabulary, for instance, identifying the main research findings, possibly with outside help, and then discussing and deciding what of the results and ideas might be worthy of adaptation and implementation, and what support might be necessary to do this.

In fact, of course, this is precisely what happened in Britain in the mid-1970s, when modern foreign language teachers were faced with the problem of how to motivate 'lower-ability' learners. The result was the Graded Objectives Movement that claimed success, and which certainly had considerable influence on national systems of examining in the UK. Interestingly enough, such movements were initially teacher-inspired and led, and rarely received national funding or support until their success was already apparent.

Teacher-centred dissemination often happens when teachers are brought together on in-service courses. Such in-service courses are designed to update teachers with the latest thinking and research in language education, and are usually funded by national governments who hope thereby to upgrade their language education programmes. Courses may also have the added advantage of being associated with language education projects that provide a direction, motivation and resourcing for change, and I shall discuss the advantages of projectisation and its associated evaluation later.

There is, of course, no simple solution to the need to make classroom teachers aware of research results, and to encourage and support them in changing their teaching towards what is thought or known to be more effective: if there were, it would have been tried long ago. I have also no doubt that all the means of dissemination and support I have mentioned are important parts of the jigsaw puzzle: action research, exploratory practice, teacher organisations, regional and national conferences, national organisations like CILT, the work of the Council of Europe, electronic networks, in-service education and writings about Reflective Teaching, the existence of groupings like Teachers Develop, Teachers Research!: all these doubtless help, all address the same general topic and need, and express the same frustration.

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The fact is that the issue we are addressing - that of getting teachers and others to reflect upon what they do, and to change, of getting research acted upon - is probably best understood within the framework of innovation theory in general, and policy-makers and researchers alike would do well to look to writers on innovation to reach a better understanding of how change can be brought about. I do not propose in what follows to go into detail on the topic of innovation, and would certainly wish to encourage readers to consult the growing literature on the topic. However, one or two remarks will hopefully not go amiss.

6. INNOVATION THEORY

Some writings on language education seem to make very simplistic assumptions about how things might be changed. In the 1970s, for example, much discussion and writing took place on the topic of syllabus design: how students' needs might best be identified and incorporated in syllabuses for teaching and learning, how language use might best be conceptualised and categorised for the purposes of teaching. The debate, indeed, continues, and rival approaches attract articulate adherents: see Breen (1987). Few references are made to empircial verification of the proposals, to evaluations of effectiveness, and the impression is often left that what matters is changing the syllabus, and much will follow automatically.

Other debates have raged around teaching materials: how they are best designed, what language teaching and learning philosophies and theories are best incorporated, and how the designs might best be evaluated. Very few research studies have taken place to date of textboooks in action (but see Bonkowski, 1995 for an exception), yet the belief remains strong that textbooks have enormous influence, and can somehow bring about significant change (publishers, of course, have a vested interest in such claims).

There is a long held belief in language teaching circles that tests and examinations are a major obstacle to change in curricula, materials and methodologies, and some, including myself, have claimed that if that is the case, then the best way of bringing about change might be to change the assessment instruments, in order to create 'levers for change' (Pearson, 1988). The fact remains, however, that getting innovation to occur is a long, hard business, and all programmatic statements about likely panaceas, or even assertions about causes for lack of change, need to be examined critically and carefully. Alderson and Wall (1993) for example, present a review of the literature into test washback - the assertion that tests impact on teaching - and find very little empirical evidence one way or the other. Wall and Alderson (1993), however, present compelling evidence that positive washback is not easy to engineer, and that simply changing an examination is unlikely to bring about the hoped-for change. Alderson and Wall conclude that much further empirical study of asserted washback is needed, and that our understanding of how tests might impact on teaching and teachers, as well as learning and learners, is only likely to be enhanced by a more refined notion of what washback is, by more empirical investigations, and by a better understanding of pupil and teacher motivation on the one hand and of how innovation and change occur on the other.

Markee (1994) provides a brief but very useful overview of writings about innovation in disciplines outside language teaching, including urban planning and sociology, and urges his

readers that "the adoption of a diffusions-of-innovation perspective should be regarded by practitioners as crucial to the development of the field of language teaching" (1994: 229). He cites Kennedy's 1988 account of the hierarchy of systems which constrain innovations: innovations at the level of classroom have to operate within institutional, educational, administrative, political and cultural subsystems, all of which will affect the success of the innovation. Rogers (1983) synthesises from some 1500 studies of innovations across disciplines a number of attributes which innovations appear to possess which may promote or inhibit their adoption. Markee cites these as follows:

" - the relative advantage to potential adopters of adopting an innovation (ie the costs or benefits)
- the compatibility of the innovation with previous practice (ie how different or similar the innovation is to what the potential adoptor already uses)
- the complexity of the innovation (ie how difficult the innovation is to understand or use)
- the trialability of the innovation (ie how easy it is to try out in stages
- and the observability of the innovation (ie how visible the innovation is)"
(1994: 236)

Fullan (1993) emphasises that innovation is a process, not an event, and that it takes much more time than is often supposed. He sets out three stages of innovation: initiation, implementation and continuation, and claims that at each point, the innovation needs to be inspected for its relevance, for the readiness of those involved to innovate, and the resources available for innovation.

From these considerations, it is clear that many advocates of innovation in language teaching only address the quality of the innovation itself, be it test, syllabus, method or textbook, and do not take account of those factors that might constrain or promote its adoption. The management of the change is at least as crucial to success as is the nature of the innovation itself, and any consideration of the effectiveness of language teaching needs to address these issues if subsequent change is desired.

7. PROJECTS AND THEIR IMPACT

One increasingly common approach to innovation in English language education in the 1980s and 1990s is the Project Approach (see Alderson in Alderson and Beretta (1992) for a brief description of this). So-called projectisation, at least from the UK, was spearheaded and sponsored by the Overseas Development Administration, as a way of focussing its investment in language education but also partly as a way of achieving greater Value for Money, since there was a prevalent belief that general support to language programmes was less effective than time-limited approaches with clearly defined and delimited aims and objectives, a statement of risks and a framework for inputs and outputs, as well as designation of indicators of achievement for the evaluation of success. Experience in such projects is fairly well documented in Dunford Seminar Reports (1988, 1989, 1992) as well as in-house reports and evaluations) and these represent a valuable resource for an understanding of how effective

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such forms of intervention might have been. Projects are of particular relevance to this Workshop, since they have been the main means whereby assistance has been provided by Britain to English language education in East and Central Europe in the 1990s. I strongly recommend the monitoring of the success of on-going projects by the Council of Europe, and member governments. Examples of projects abound and details are available from the British Council ELTECS scheme.

Clearly such language education projects do not constitute research projects per se, since they are principally aimed at the implementation of a programme of (usually innovatory) action. Equally clearly, even if the effects of such projects could be unambiguously established - which, as we shall see below, is difficult - it would be virtually impossible to establish cause and effect relationships between input and outputs, or between outcomes and the multiplicity of factors that conditioned the provision, modification and omission of support in various forms to the projects. Thus, generalisability of the experience of one project to other contexts or sectors or projects is difficult if not impossible, and so the relevance of the project experience and evaluation may be of limited extent. Nevertheless, projects do offer instructive examples of interventions in language education from which we could learn a great deal, and which will doubtless in the next few years contribute substantially to our understanding of innovation in language education.

One particular feature of recent project work has been an increase in the systematicity whereby projects are planned and evaluated. Two features stand out in the literature and in practice: the conduct of baseline studies to establish project contexts and objectives as well as the status quo ante, and the design of impact studies to assist with the evaluation of project effects.

Baseline studies are typically conducted before the inception of a project, or before its main design features have been established. They consist essentially of surveys of existing practice in the relevant sector, and are usually used to guide project design or project implementation, progress and policy. In addition, if appropriately designed, they provide an excellent basis for comparison of subsequent effects: impact studies.

Impact studies are designed to establish what effect projects or other interventions have had. They necessarily require some form of statement of the status quo ante, which is why baseline studies are frequently so crucial in the design and interpretation of impact studies. Ideally, an impact study will set out to identify not only the planned impacts a project has had or is having, but also to establish any side-effects: unplanned or unanticipated spin-offs. Effects to be expected include pupil learning outcomes, enhanced teacher competence, enhanced teacher and pupil motivation, greater provision of resources, better facilities, changing teaching methodologies, improved examinations and tests, and the like. Unexpected effects might be positive or negative and might include things like increased status for language teachers, increased stress and workloads, greater employability of graduates from the project, impact on teachers of other languages within the system, and so on.

Data is beginning to emerge from impact studies that will inform our knowledge about what effects projects can and do have, and what effective language teaching might look like. For example, an ongoing impact study in Nicaragua (Luxon and Luxon, 1995) has shown that teacher education has resulted in massive increase in the use of the target language in classes, in greater use of group and pairwork activities, and in a greater variety of classroom activities than had previously been the case (as established by a baseline study).

8. EVALUATION

The advent of the Project Approach came at roughly the same time, and indeed may well have been due to the same causes, as an increase in interest and activity in evaluation in language education. Although widespread in North American education generally in the 1960s, and in Britain in the various curriculum development projects that burgeoned in the 1970s, evaluation of language education programmes had been relatively unusual until recently. Certainly the work of the Assessment of Performance Unit (APU) informed British foreign language teaching in the 1970s and 80s, but ELT, and EFL in particular was notorious for its lack of attention to evaluation. But increasing concerns for Value for Money led the ELT profession finally to take evaluation more seriously in the 1980s, and this in turn had a knock-on effect on publications. Even today, much evaluation is seen by practitioners to be threatening. Many writers are at pains to emphasise the formative role of evaluation, and to de-emphasise summative aspects.

Nevertheless, despite such suspicions, the need for evaluation to be taken seriously is increasingly recognised, and the language education profession can learn a great deal from project and programme evaluations. All the more reason to regret, therefore, that most evaluations result in in-house reports rather than published accounts. If policy makers and language educationalists are to benefit from experience, it is important that such reports be published, and that lessons be regularly drawn, and disseminated, from the experience.

One aspect of project and programme implementation that does not always emerge from evaluation reports, however, is the importance to project success of the quality of the people involved in the project, and their interrelationships. Designs can be made by experts in relative isolation from the real world, but they have to be implemented by individuals, who have their own perspectives, agendas, motivations and understandings. The quality of the personnel, their ability to manage, administer, delegate, teach, listen, learn from each other, tolerate, recognise and correct mistakes, adjust activity to suit other people and changed circumstances: all these qualities crucially affect how well plans are implemented. Often it is difficult either to isolate, or even to acknowledge such matters, but most people who have worked on or with projects will readily tell 'war stories' of projects they have known that collapsed because of individuals' commitment or lack of it, that succeeded because of particular individuals' enthusiasm and commitment, that changed direction because of new members, or changing relationships among team members.

Thus the effectiveness of language education is often less a matter of design, methods and materials, than it is a function of the human relationships involved. Although teaching is a

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lonely activity, working on projects and programmes is often a matter of getting on with other, often difficult people, and the ability of individuals to do so, and deliver what they are required to at the end of the day, varies enormously. People need training in project management and project participation, especially when such projects involve cross-cultural exchanges and relationships, as is often the case in language education. Policy-makers need to consider such issues very carefully when planning or evaluating projects or when drawing the lessons from successes and failures.

9. SUCCESS AND OUTCOMES

Even a brief discussion of evaluation and mention of 'successes and failures' leads necessarily to the question: how are we to define success, be it of a project or programme, or of an individual teacher or learner? In one sense, at least for projects, success is easy to define, since it must relate to the achievement of the objectives the project has set for itself: the rationale insists that we measure success in terms of clearly defined objectives. Whilst the measurement itself may be difficult, what is to be measured is usually defined by the project framework, and its indicators of achievement. Good projects, in other words, consider how they are going to evaluate their success before they are implemented.

However, for most language education programmes, such clear definitions are not usually available. Although it is commonplace to talk about linguistic and non-linguistic outcomes, in practice these are defined in default only by the public examination system, if one exists. Otherwise it is assumed that teachers will have their own notions of what it means for learners to be successful. Since teacher definitions of success are likely to vary enormously, most systems have developed some form of national or regional examinations, which may include a measure of coursework or class-based assessment also. Much store is set by such public systems - they act as gatekeepers for higher education, as qualifications for employment, as well as indicators of school achievement. Thus it may seem reasonable to use performance in such examinations as an indicator of the success of a programme, project, or individual, and of the effectiveness of teaching. Unfortunately, there are severe problems in so doing.

Firstly, many desirable linguistic outcomes may not be measured in such examinations: most commonly, oral communication abilities are rarely included, and when they are, are frequently measured in limited and often unreliable manner. Secondly, examinations are usually not comparable in difficulty from one year to another - or at least, belief in their comparability is an act of faith and is not empirically established. Thirdly, many public examining bodies do not have standards of validity and reliability to which they adhere, or even codes of practice which would guarantee the quality of their examinations, as a recent survey by the International Language Testing Association has established (Davidson et al, 1995, and also the Survey of British EFL Examination Boards in Alderson and Buck (1993) and Alderson et al, 1995). Many examinations are frankly very poor measuring instruments, and at the very least, policy makers should insist that examining bodies be made publicly accountable for their results and their measures according to the commonly accepted standards of educational measurement. It is worth recording here as an aside the efforts of ALTE - the Association of Language

Testers in Europe - to develop a Code of Practice which should result in considerable improvement in language examinations. However, most national examination bodies are not members of ALTE!

As a result of the inadequacy or irrelevance of public examinations, many researchers are obliged to develop their own measures of success, and I would argue that this is generally quite appropriate especially when the research is focussing upon a particular aspect of language ability or achievement. However, this creates a lack of comparability across research studies, and makes it harder to identify or interpret trends in research. Unfortunately, moreover, all too few researchers know how to develop appropriate measuring instruments, and few research studies report the reliability and validity of their outcome measures: the quality of the instrument - usually a test - is assumed, or asserted, but rarely empirically established.

Language testing is all too often seen as a discipline separate from language teaching, as if it had nothing to do with it: Johnstone's own annual survey of research in Language Teaching deliberately ignores research in language testing

"I have avoided the incorporation of FL/L2 assessment as such in this review because the field is too large" (1993:134).

Again, this is an issue to which policy-makers need to turn their attention, since ignorance of language testing and assessment results in ill-defined standards of achievement, inadequate measures and ultimately a deep lack of knowledge of what it is that language teachers are achieving.

Some progress has recently been made towards remedying this lack of agreed standards for levels of achievement. The English-Speaking Union's Framework is an attempt to establish the relative levels of different British EFL examinations, and ALTE is also working towards defining comparability of examinations across national systems. In England and Wales, the advent of the National Curriculum, and the defining of 10 levels of attainment, to be measured at ages 7, 11, 14 and 16 has certainly improved the previously chaotic system, and introduced the notion of national standards into the educational debate. However, the levels as set out in various documents are not empirically based: they do not take account of what is known about progression in language achievement, and are therefore naive in assuming some sort of linear progression, as Johnstone has pointed out:

"The stages and levels of the National Curriculum are based on the assumption that progression is akin to climbing a ladder. While it is true that variability is allowed for in that a pupil may be on different rungs for different purposes eg listening, and speaking, reading and writing - very little account is taken of variability in performance according to task-related and other factors....Until a clearer picture of the nature and scope of variability emerges, then a question mark must be put against the validity and reliability of any standardised national scheme based upon a simple model of upwards progression". (1992: 141).

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In point of fact, language testers have long recognised variability according to task - often called 'test method effect' - and have gone to great lengths to develop appropriate test specifications for given levels and to research into task difficulty and its causes. Unfortunately, policy-makers have ignored this research in their drive for unified and inevitably over-simplified national 'standards' of achievement.

Nevertheless, I believe that national tests based upon reasonably defined levels of expected achievement have considerable research potential, provided that the tests themselves are developed according to the usual psychometric criteria. Researchers should be prepared to examine performance on such tests to help understand how and in what areas pupils do progress. We should not, however, use such tests to prescribe what pupils should learn until we know better what they do learn, much less to judge teachers and schools on the basis of such test results.

Success in language learning and teaching must at least in part be defined according to purpose, age, and context. Success is inevitably relative, not absolute, and will be defined differently within different systems, as the IEA studies acknowledge, and by different participants - pupils will have different views from teachers (hence the increasing interest in the validity and value of self-assessment), and employers will have different perspectives from teachers. Provided that we acknowledge the plurality of valid definitions of success, and the need for validity and reliability in our definitions, it is perfectly possible to make progress in our measures of linguistic success.

Other outcomes are, however, also important to language education, and here we have made less progress, whilst acknowledging the need for the measurement of non-linguistic outcomes. The fact is that in the literature, such as it is, on non-linguistic outcomes, there is little operational definition. Too few researchers and writers about such matters ask themselves what effects are measurable and quantifiable, and what effects are elusive. We ought to be not only stating what impacts are desired from effective language teaching, as happens in much literature on language and cultural awareness: we ought also to be defining what such expected outcomes look like, and devising appropriate measures to identify them. It is not the purpose of this paper to attempt such operational definitions, but to draw attention to the need for greater attention to their measurement and to improvements in the means we use to identify and observe them.

Attitudes to language, to language learning, to speakers of the target language and to the target culture, have long been considered important. Byram (1991) draws our attention to the possibilities for increased cultural experience and cultural awareness, a realisation of cultural relativity, and a critical attitude to own culture and assumptions, and describes the development of learning programmes to achieve this (see also Byram, 1992). Friendships with speakers of other languages are surely important non-linguistic outcomes, as are the ability to avoid cross-cultural miscommunication, the ability to tolerate each other as well as speakers of other languages, the ability to work with people of different learning styles, personalities, and levels of achievement, the ability to work in teams, to clarify one's own goals, to solve problems, to engage in critical thinking. Many of these desirable outcomes are, of course, not

confined to language education, but might reasonably be expected of it: much discussion is currently being held of the need for education to define and inculcate transferable skills, core competences, and even things like good citizenship, environmental awareness, and the like.

Closer to language proper but not necessarily considered as linguistic outcomes are things like language learning strategies and study habits. It has been claimed that learning to read or write in one language can transfer to another, both to and from one's first language (although the research evidence, yet again, is ambiguous on this). Research into language immersion programmes, such as the well-known Canadian studies, has shown that subject knowledge is acquired through programmes intended to improve language skills, and that bilingual education does not impede the development of pupils' other knowledge and abilities. Content-based language teaching is frequently advocated and practised in various parts of the world. One might then reasonably expect improved subject matter knowledge from language education, too.

However, whilst acknowledging that all these non-linguistic outcomes are imaginable, desirable and even crucial, a note of caution is in order: we are good at developing ideas for the value of language education. We are much weaker when it comes to showing that such laudable benefits are in fact achieved by the programmes we develop, and much more attention needs to be paid to developing our measures and to showing the value of foreign language education. We are still not very good at developing measures of linguistic outcomes, much less of non-linguistic outcomes. Policy makers take note.

10. RESEARCH

And so finally back to research, and what we know about the effectiveness of foreign language teaching. My colleagues in this Workshop will have much more to say on this than I do, but I should like to add one or two thoughts of my own, as well as to repeat in a slightly different context some of the things said above.

The dependent variable in much research is frequently undefined or ill-defined, or not considered to be problematic. Most attention is usually given to the independent variables, and as a result, it is often difficult both to compare results from different studies, and even to accept the importance of the effects being claimed. All too often, paper and pencil measures are used instead of measuring learners' ability to process and produce language - structures, or whatever - in real time. A very limited view of language proficiency often prevails, despite advances in our theoretical understanding of the complexity of the construct. For instance, the otherwise valuable European Language Proficiency Survey (Coleman, 1996) is limited in its significance by the use of only one, somewhat controversial, measure of proficiency (the C-test) and claims based upon such measures need to be examined critically.

Unfortunately, much published research is inadequate in other ways, not just the Suggestopaedia research mentioned at the outset of this paper. Gregg (1984) has drawn our attention to the problems of much of Krashen's work empirically as well as theoretically;

Griffiths and Sheen (1993) have similarly criticised research into field independence/ dependence, for inadequate consideration of constructs, and poor operational definitions. In such cases, even the independent variables are poorly defined.

In part, no doubt, we can blame the pressure on academics to publish, for the sake of their jobs or their institution, and the ambition of some to make a name for themselves or to make money. There is doubtless also much pressure for research to come up with results relevant to policy makers, in order to get continued funding.

In part, we can also blame the naivety of the policymakers themselves who appear to believe exaggerated claims or simplistic statements - the story of the Army General who read on an airplane about the achievements of Suggestopaedia, and insisted that the Defense Language Institute in Monterey develop a large scale implementation of the method is, I am assured, not apocryphal.

However, if we want our research to be acted upon, and therefore to be relevant to policy, we need to know what sort of questions policy-makers ask or need answered. Our research needs to be user-friendly to a number of different users, not just to other academics.

Much has been made in the last decade or so of supposedly new research paradigms. Whole issues of journals like TESOL Quarterly and Applied Linguistics have been devoted to qualitative research methods and their implications, more and more researchers advocate 'ethnographic' methods, and it is common for researchers to criticise some research for being 'quantitative', overly concerned with statistics, or experimental manipulation. However, the distinction between qualitative and quantitative research is a false dichotomy: the two traditions are not opposed to each other, nor indeed are they so different. Quantitative researchers should always be concerned with the quality of the instruments - that is the meaning of validity - and they necessarily engage in subjective interpretation of the results they gather. Qualitative researchers make quantitative statements by using adverbs like often, occasionally, and so on - and ought to be more aware that they often seek both to generalise as well as to identify exceptions. Instead of wasting effort on criticising other research paradigms, we ought to see other traditions as complementary to our own, and to triangulate wherever possible: statistical analysis of data should be accompanied by descriptive accounts of events and responses, and narratives of events or perceptions ought to include careful statements of how typical or atypical the results are, and how believable. I very much hope this Workshop will not engage in such futile activities as criticising other paradigms, but will accept the need for a plurality of methods to increase our understanding of the phenomena that concern us.

Having said all the above, I do not wish to finish on a negative note. What DO we know about the effectiveness of foreign language teaching?

Research into second language acquisition has blossomed over the past two decades or so. Ellis (1993) discusses its contribution to ELT: we now understand better that what learners process in classrooms is not necessarily what teachers and materials writers intend. Therefore

more important perhaps than the question: "what is the best way to teach?" is "What conditions can we create that facilitate learning?". He criticises the PPP - presentation, practice and further practice - approach to teaching, and especially to grammar teaching, and suggests that research shows that learners will acquire structures better if they are 'pushed' to make their output more comprehensible. He suggests that SLA can contribute to an understanding of what conditions lead to more effective grammar learning in classrooms, and lists "focused communication activities, consciousness-raising tasks designed to get learners to construct their own explicit grammar, and the notion of interpretation grammar tasks that get learners to listen to input and interpret it" as compatible with what SLA research has discovered about how learners learn grammar.

However, in this renewed interest in grammar, it is important to ask "what grammar?" A return to traditional grammar - something often advocated by policymakers and politicians, because they believe it used to work for them - is not recommended. Alderson et al (1995b) provide strong evidence that knowledge of grammar defined as being able to identify and label parts of speech, and state grammatical rules, shows virtually no relationship to levels of language proficiency, at least for learners of French at UK universities. Interestingly, this study also calls into question the practical value of that aspect of Language Awareness called metalinguistic knowledge, something which university teachers of French in the UK, at the very least, need to be aware of.

Johnstone (1992) lists research issues that SLA has explored in its contribution to a better understanding of language education: the nature of input, the role of negative feedback, of explicit grammar instruction, and of positive modelling. However, he also acknowledges that debate rages among researchers as to the precise nature and contribution of these various variables and suggests that conclusions can only be tentative at this stage. Although the advocates of what he calls "instructed acquisition" appear to have the widest acceptance, it is far from clear that language instruction does indeed help in all circumstances, although White, Spada et al (1991) conclude that form-focussed instruction helps. However, other researchers contend that even such common activities as teacher correction of error may not be as effective as believed (Schachter, 1991 Kepner, 1991, Johnson, 1985, Makino, 1993, and other researchers have explored what sort of feedback might provide the most valuable information. Doubtless much depends upon the nature of the instruction. How valuable classroom interaction might be, the negotiation of meaning with peers or with the teacher: such issues remain unresolved, and the object of continued study.

Even if the jury is still out on the question of whether the classroom is of any use in language acquisition, and if so, what use exactly, it is clear that exposure to the language is important, and that learners need to use the language in order to make much progress. We know that spending time in the country where the target language is spoken has an effect: it enhances both language proficiency and non-linguistic aspects like increased understanding and awareness. This used to be a tenet of belief, but now we have some research into the topic (under the heading of The Year Abroad) to help us refine our beliefs. We now know that simply sending students overseas is no guarantee of success: they need to be well prepared for their visit. Students will benefit more from work placements than from study, they need

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actively to seek language use opportunities and to avoid speakers of their own language, they need to be aware of the risk of increased intolerance for the other culture as a result of inadequate contact or comprehension of the culture.

No article on the effectiveness of language teaching would be complete without some consideration of the effects of technology. In fact, there are very few substantial studies of the matter: most are short, one-off studies, and it is impossible to conclude anything about effectiveness. There is evidence from Canada that immigrant children learn writing better by using computers. A study in America of English speakers learning German at university with computers (Chun, 1994) suggests that writers feel less inhibited, use more questions, and evaluate each other more. Interestingly, since the computer collects the data automatically, data collection is very straightforward. The use of computer-assisted learning is particularly interesting for this reason, and the study of learner involvement with tasks, and data on their thought processes and language development, may be more easily studied through this medium.

There are also suggestions in the literature that learner autonomy might be encouraged through the use of computer corpora: allowing learners to explore the language data themselves frees them from the teacher, and allows them to develop their own hypotheses about linguistic rules and the use of the language. Once more, speculations abound, but hard evidence of effectiveness is elusive.

However, it is likely that language learning without technology can be just as effective as with. After all, language learning was effective long before computers came on the scene. Recent research into the use of extensive reading, for example, emphasises the role that reading enjoyable texts in class can play in providing exposure to the language and developing both reading strategies and the habit of reading for pleasure in the foreign language, which is likely to be not only motivating, but also reinforcing of language proficiency. Perhaps teachers would be more effective if they reduced the amount of interference they create in learners' engagement with the language, and allowed them space and time simply to read and enjoy using the language.

There is a pervasive, common-sense belief in language education, and in education generally, that nothing succeeds like success, and that failure breeds failure. This belief emphasises the importance of motivation in language learning, and there is a long history of research in this field. Interestingly a recent revival of interest is resulting in the important insight that motivation is in the learning situation not in the learner alone (Young 91, see also Dornyei, 1994a and b). From a distinction between integrative and instrumental motivation, research has now developed the understanding that these two are not opposite types of motivation, but complementary: both can contribute to learning. Oxford and Shearin (1994) list the following reasons their subjects gave for learning Japanese: intellectual stimulation, seeking a personal challenge, enjoying the elitism of taking a difficult language, showing off to friends, developing greater cultural tolerance, aiding world peace, satisfying curiosity about cultural 'secrets', fascination with Japanese writing systems, and having a private code their parents would not know. The complexity of motivation is clear from such a list, as is the need for

better theories, and further research into which of such motivations might best sustain language learning

However, what may matter most is not so much the learner's intrinsic motivational state but the interaction between the learner and the learning setting. Learning tasks themselves have a motivational effect, as do teachers, materials and other elements in the setting. If learners find the experience enjoyable they are more likely to learn and they are more likely to find the experience enjoyable if they succeed in the task. However, tasks that are too easy are not necessarily motivating: learners who find a task difficult engage in deeper-level processing, which leads to better retention, at least of lexis (Laufer and Osimo, 1991). Since task difficulty will vary according to the individual learner, it follows that what motivates one learner in a class will not necessarily motivate another. Equally, since some learners are more persistent in their efforts, and some are more influenced by fear of failure, or by peer evaluation, whereas others have higher levels of aspiration, individual variability in motivation in the engagement with learning tasks is to be expected.

The converse of motivation is anxiety and recent research has revealed something of the sources and effects of anxiety (Young, 1991, MacIntyre and Gardner, 1989, 1991, 1993, Horwitz and Young, 1991). Anxiety is held to reside more in the language learning experience than in the learner, and especially in situations when learners might make use of the target language. The more steps that teachers and learners can take to reduce such anxiety, the more effective, presumably, the teaching is likely to be.

Just as individual differences in motivation and aptitude for language learning have long been known to be important in success, variability in performance on tasks has been known in language testing, and recently found to be important in SLA. Current research seeks to tease out the conditions which makes some tasks more effective than others (see the reference to Ellis, op cit). For what it is worth, my own belief is that the most profitable research lines that will be followed in the next few years relate to language learning tasks: their nature, conditions that make them effective, difficult and motivating, and how teachers can best facilitate learners engagement with them.

This will combine with greater understanding of what actually happens in classrooms. Classroom learning research is crucial to an understanding of effective language teaching, but it has not yet yielded much positive insight. The picture that emerges from many classroom studies is rather depressing: a great deal of teacher talk, minimal interaction among pupils, imitation and memorization of complex language forms (see, for example, Weinert, 1994) Meara (1993) analyses the Nuffield Survey Questionnaire results to show that little has changed in the teaching of modern foreign languages at least at UK university level in 30 years. Alderson and Hamp-Lyons (forthcoming) suggest on the basis of a study of Americans teaching towards the TOEFL, that teachers seem to hold very naive learning theories: if learners are exposed to rules about the language, they will learn; practice is essential and should be carefully controlled by the teacher; input must come from the teacher; test-taking will lead to learning. If language teaching is to make progress, we need more studies of how teachers actually teach, and why they behave the way they do.

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Interestingly, this increasing realisation that individual learners vary enormously, that individual tasks can have quite different effects, that teachers teach in very different ways, and that most teaching as well as most innovation is locally conditioned and constrained: all these insights that have come upon us in the past decade and more point to the 'Death of the Method', as Allwright once called it. The notion of Method implies a homogeneity of methodology, of teacher, of learner, and of context, that simply does not reflect reality, or the results of the research studies that have been published: what works in one setting for one group, person or learner will not necessarily work - or even be the same thing - elsewhere.

All surveys of research end with a call for more research: more replication of one-off studies, and above all more longitudinal research, looking at learner progress over time. Such research is usually only possible with adequate resourcing, and the cry for more funds for research is perennial. However, this paper, whilst echoing and supporting such appeals, will finish on a slightly different note: research is necessary not just for its own sake, nor because foreign language learning is important and there are many unanswered questions that surround issues of effectiveness. It is also important in order to reveal the complexity of the task. Foreign language learning takes time, and involves enormous individual investment. Yet it is an example of one area of human achievement and endeavour where unresearched beliefs and prejudices are curiously pervasive, and where unsubstantiated claims abound. We owe it to our teachers and learners to emphasise that we do not have all the answers to what are complex issues, and to resist simplistic conclusions and generalisations about the effectiveness of language teaching and learning.

Will the newspapers of 1996 or beyond continue to make unsubstantiated and unsubstantiable claims for courses, methods, materials? I fear so, but I also hope this Workshop can contribute to making such claims self-evidently ridiculous to any layperson or teacher who hopes for miracles.

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